

*BEHAVIORAL PHARMACOLOGY IN BRITAIN:  
A BRIEF HISTORICAL REVIEW*

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Devotees of the 19th-century British novelist Wilkie Collins have enjoyed the surge of recent reprints of many of his long-unavailable works, as colleges (especially in North America) have incorporated Collins into their courses on the English novel, particularly in the context of the genre of detective fiction, of which he is regarded as a pioneer. Siegel (1983) has pointed out, however, that Collins could also be said to have played a distinctive, if somewhat personalized, role in the development of British behavioral pharmacology. He long took regular and ever-increasing doses of laudanum, initially to relieve the pain of gout, and he eventually came to write in such a dissociated state that he claimed not to be able even to recognize his own narrative unless he returned to the drugged state. This phenomenon of state dependency provided the central feature of Collins' most famous (but not his best) novel, *The Moonstone*, published in 1868: A man who had hidden a gem after being drugged was enabled to recall where he left it only after taking the drug again.

Despite such auspicious omens, behavioral pharmacology has had a faltering history in Britain. This is not because the study of the effects of drugs with behavior as a dependent variable has lacked vitality or force. On the contrary, one might readily develop the argument that British psychopharmacology and neuropharmacology have achieved international excellence. However, the dominant interpretive framework for such studies has been that of reductionist mechanism, with behavior being seen largely as a dependent variable for investigating pharmacological mechanisms. The experimental analysis of the behavioral determinants of drug action has not been given the interpretive priority favored by Thompson and Schuster (1968), Thompson (1984), Branch (1984), and many readers of the *Journal of the Experimental Analysis of Behavior*. In this more behavior-analytic tradition, a number of British researchers have

made important experimental and theoretical contributions to behavioral pharmacology over the last three decades, but, sadly, not always in Britain, and also without the support of sustained and effective research networks.

It is perhaps puzzling that despite the international prominence of many of its practitioners in Britain, the tradition of behavior analysis has not been more forceful within British psychology generally. One might have supposed that the empiricist tradition of British philosophy, allied to the precision of the 20th-century linguistic movement centered on philosophy in Oxford, would have provided a conducive climate for the functional analyses of behavior associated with the theoretical stance of B. F. Skinner. It is true that experimental psychology (as opposed to, say, social psychology) has always enjoyed high status in British psychology. However, the modes of explanation favored have centered on a mechanistic or so-called "cognitive" approach, of which Donald Broadbent has been a leading and articulate exponent: Behavior has been seen quite clearly as an appendage to mechanisms at some other level within the organism, which are represented conceptually by schematic diagrams with boxes connected by arrows. To this day, British experimental psychology contains a vigorous form of "cognitive neuropsychology" that does not address cognitions, does not investigate neural mechanisms except by inference, and deprives psychology of many exciting applications relevant to everyday life. Pharmacological reductionism fits readily into such an explanatory framework, of course. This perhaps explains the success of psychopharmacology and neuropharmacology in Britain rather than of an approach of behavioral pharmacology rooted in a more behavior-analytic tradition. It is, however, this latter that forms the focus of this paper.

These tensions are to be found in the seminal volume, *Animal Behaviour and Drug Action* (Steinberg, 1964). This book was based

on pioneering symposia held in London and sponsored by the Biological Council and the Ciba Foundation. It remains a remarkable volume, with a diversity of theoretical approach represented. One section focuses on neurophysiological and biochemical correlates of the behavioral effects of drugs, and includes contributions by P. B. Bradley, J. Bures, R. W. Russell, and L. Stein. Another section reviews the relevance of the behavioral effects of drugs in animals to their effects in humans. Of particular interest to the present discussion, however, are sections on "the behavioural analysis of drug action" and "factors which modify the effects of drugs on behaviour." The first of these sections includes a forceful review of the effects of drugs on operant behavior by L. Cook, who argued, "There is a growing feeling that investigators should . . . carefully specify the experimental contingencies when analysing the interactions of drugs and behaviour" (1964, p. 23). Cook reported the effects of minor tranquilizers on schedule-controlled behavior and on behavior suppressed by punishment. The second of these sections contains a characteristically lucid paper by P. B. Dews presenting further experimental analyses of schedule-dependent drug effects, with allusions to the so-called rate-dependency hypothesis that was to provide a focus for so many investigations in behavioral pharmacology in the following years. Dews trenchantly observed that

The attributes of schedules that influence the resulting behaviour are not eliminated by ignoring them. . . . Almost all experiments intended to show a difference between behaviour maintained by aversive stimuli and behaviour (maintained) by positive reinforcement in sensitivity to a drug have involved concomitant change of motivation and *schedule*. Since schedule change alone can change sensitivity it is gratuitous to attribute the observed differences to differences in motivation. (Dews, 1964, p. 201)

The alert reader will have realized that this discussion of the contributions of Cook and of Dews to this important symposium stretches the concept of behavioral pharmacology in Britain. The symposium was held in London, but Cook traveled from Philadelphia and Dews from Harvard Medical School to participate. However, it is instructive to see the context of their contributions. The

behaviorally oriented themes that they developed were taken up and elaborated in the discussions at the symposia by two researchers then working in the tradition of the experimental analysis of behavior in Britain, namely H. M. B. Hurwitz (soon to leave Birkbeck College London for Tennessee and subsequently Guelph, Canada) and D. P. Hendry (soon to leave Durham for Illinois). W. H. Morse, from Harvard Medical School, also made effective contributions in this vein. It is interesting to note that Morse spent the academic year 1963–1964 in the Royal College of Surgeons of England in London, working to establish a small behavioral pharmacology laboratory with John Vane, who was subsequently awarded a Nobel Prize for his pharmacological research. Morse was followed in succession by Eliot Hearst (1964–1965), Eve Segal (1965–1966), and Lew Golub (1966–1967). Interests in behavioral pharmacology in the behavior-analytic tradition do not seem to have thrived in this laboratory after that, despite such a considerable infusion of talent from the United States in establishing a laboratory well versed in behavioral principles.

There were other contributors at the Ciba symposium who at least appeared to be sensitive to the empirical importance of the behavioral determinants of drug action, including S. A. Barnett (Glasgow, later to go to the Australian National University in Canberra), L. Weiskrantz (Cambridge, soon to move the few miles to the Chair of Psychology at Oxford), and H. Steinberg (University College London). Steinberg's own presentation at the symposium (Rushton & Steinberg, 1964) included data on the modification of drug effects on behavior by prior experience, a research program that, though not based on the controlled techniques of operant conditioning, nevertheless anticipated more recent interests in the effects of history (e.g., Barrett & Witkin, 1986). A paper by C. R. B. Joyce (London Hospital Medical College) also emphasized the importance of recognizing behavioral determinants of drug effects. But by far the greater force at the symposium, both in terms of formal presentations and unscripted discussions, was attributable to a less behaviorally oriented approach. For example, general discussions were devoted to "the biochemical approach," "the electrophysiological

approach," and "the neurophysiological approach," but not to the behavioral approach represented so cogently in the papers by Cook and Dews. Indeed, some contributors were clearly unpersuaded by the importance of behavior analysis. For example, Irwin (1964, p. 276) briskly listed eight of what he saw as the "many limitations" of "bar-press operant conditioning." These included the undue experimental effort required, the unnecessary complexity of the experimental procedures employed, and even, perversely, the fact that "the direction and magnitude of response to drugs is very much dependent on the schedule"! Dews must have blanched at this determination to believe that somewhere there is a "true" effect of a drug on behavior untrammelled by the noise of different forms of environmental control. Be it noted, however, that Irwin too was a visitor to Britain from the United States to participate in the symposium!

The Ciba symposium is instructive today not just because it was the first major event in Britain at which the new behavioral pharmacology was presented and discussed. A number of themes can also be identified in the pages of the proceedings, and a number of research pedigrees can be traced from the participants.

One theme already mentioned is the loss of talent in the experimental analysis of behavior from British psychology. In the decade during which behavioral pharmacology was systematically developing in the United States, Britain lost not only Hurwitz and Hendry but also Davison (from University College London to Auckland, New Zealand), Gilbert (to the Addiction Research Foundation in Toronto, Canada), Millenson, Harzem (from Bangor to Auburn University), and Stretch (from Queen's University Belfast to Alberta and thence to other posts in Canada). R. G. A. Stretch deserves special mention, for he was the first researcher based in the United Kingdom to contribute data in the full Skinnerian tradition to the literature of behavioral pharmacology. A paper in the *Journal of the Experimental Analysis of Behavior* (Stretch, Blackman, & Alexander, 1966) reported schedule-dependent effects of methylphenidate on behavior maintained by avoidance schedules, and a paper in *Nature* (Stretch, Blackman, & Bradley, 1967) reported sched-

ule-dependent effects of pentobarbital on food-maintained behavior, in both cases using multiple schedules and within-subject experimental designs. Stretch was an exciting psychologist. When I went to Belfast in 1964 (on the recommendation of the head of my undergraduate department R. L. Reid, a committed Skinnerian and long-time personal friend of Skinner), Stretch was excitedly unpacking his three new Grason-Stadler rat boxes and associated electromechanical control modules. Stretch conveyed to me his tremendous but then still untested enthusiasm for the experimental analysis of behavior, insisting on detailed discussions of Murray Sidman's *Tactics of Scientific Research* and encouraging me to write to Sidman and to Joe Brady for reprints (both of whom promptly sent voluminous collections of their work that I have retained in special files to this day, supplemented regularly, of course, by their subsequent publications). I never stopped to ask myself where Stretch's own understanding and experimental skill had come from, or who had inspired him, but I think he was entirely self-taught. His experimental "green fingers" were a direct reflection of his burning enthusiasm for this different approach to experimental psychology. Behavioral pharmacology would, I am sure, have been much better developed in Britain to this day if Stretch had resisted for a few years the lure of more prosperous and supportive research climates abroad. In Canada he made early contributions to the experimental analysis of the response-produced shock phenomenon and studied the effects of drugs as reinforcers before his tragic death in a boating accident.

The research pedigrees that can be traced from the Ciba symposium are diffuse, and I hope that the following very selective review, based on a definition of behavioral pharmacology emphasizing the fundamental importance of the behavioral determinants of drug action, will not give offense either by omission or by commission. First may be traced what has been called "the London tradition." Hannah Steinberg herself formed the principal focus of this line, reporting the results of systematic experimental investigations of drug synergies as well as the effects of prior pharmacological and behavioral histories. Steinberg was subsequently appointed to the first designated Chair in Psychopharmacology

in Britain. Among those she supervised may be mentioned I. P. Stolerman, who subsequently moved to Bradley's neuropharmacology laboratory at Birmingham University and whose present post is at the Institute of Psychiatry in London. Although also pharmacologically sophisticated, Stolerman has made major contributions to behavioral pharmacology in the behavior-analytic tradition, being perhaps particularly well known for his work on the effects of nicotine, taste aversion, and the stimulus properties of drugs (see, e.g., Stolerman & D'Mello, 1981).

D. J. Sanger was also a doctoral student of Steinberg's in the early 1970s. He then took a postdoctoral research fellowship awarded to him by I.C.I. (Imperial Chemical Industries, a major British pharmaceutical company) to the operant conditioning laboratory in the Department of Psychology at Birmingham University (to whence I had moved via Nottingham) and began a remarkably effective and sustained program of experiments on the rate-dependent effects of drugs (e.g., Sanger & Blackman, 1975, 1981) and on the effects of drugs on schedule-induced behavior (e.g., Sanger & Blackman, 1978). Robbins and Stolerman (1990) have recently described these programs as "a British counterpart to the American 'hard operant' school," a comment that I, at least, regard as a considerable compliment! Sanger and I then moved to University College Cardiff in 1976. Although the research programs were successfully transferred to a new laboratory, it proved impossible for some time to gain further financial support from the Research Councils in the U.K. (except in the form of an occasional funded doctoral student), reviewers of applications asserting that they lacked theoretical context (meaning that they were addressed to the theoretical interests of behavioral pharmacology in the behavior-analytic tradition rather than to assumed underlying "explanatory" mechanisms). As an inevitable result, Sanger left university-based research for a research post in a pharmaceutical company in Hull, and then later took the further step of leaving Britain for a research post in a pharmaceutical company in Paris, where his continued research output again draws attention to the loss of talent that British behavioral pharmacology has been forced to endure. Moreover, a particularly promising

doctoral student in the Cardiff laboratory with Sanger and Blackman in the early 1980s, A. J. Greenshaw (e.g., Greenshaw, Sanger, & Blackman, 1981) promptly took a post in Canada on obtaining his doctorate, where he too has prospered scientifically, although with an increasingly pharmacological orientation (e.g., Greenshaw, Baker, & Wishart, 1989).

Another pedigree that can be traced from the Ciba symposium is to be found in those centers of excellence in British experimental psychology, Oxford and Cambridge. A focal figure here is Weiskrantz. He was (and is) by no means an advocate of Skinnerian behavioral analysis, being, of course, a distinguished experimental neuropsychologist (in this case genuinely so, for his experimental research has addressed the effects of brain damage on memory and vision). Weiskrantz's research, however, has demanded behavioral sophistication (as, for example, in testing behavioral deficits in operated monkeys) and his behavioral sophistication was shown, for example, in his excellent edited text, *Analysis of Behavioral Change* (1968), in particular through his own contributions to that book. At Cambridge, Weiskrantz provided intellectual support for S. D. Iversen, who returned from postdoctoral work in P. B. Dews' laboratory at Harvard Medical School. The text on behavioral pharmacology, written by Iversen and her distinguished neuropharmacologist husband L. L. Iversen (1975), gives full priority to the importance of the behavioral determinants of drug effects, with lucid expositions of the techniques of the experimental analysis of behavior and of early work on schedule-dependent drug effects, allied to similarly clear exposition of basic neuropharmacology. Together the Iversens brought together behavioral sophistication and neuropharmacological expertise of high order. From the perspective of behavioral pharmacology as defined in this article, it is perhaps a pity, though it was probably inevitable, that their research interests drew them increasingly to neuroscience of a traditional kind and thus away from the lower status afforded to a behavioral approach by British science. They subsequently also left university research laboratories in the early 1980s to establish the relatively well-funded and now already powerful Neuroscience Research Centre founded by Merck, Sharp, and Dohme near Cam-

bridge. Weiskrantz had by then also left Cambridge. However, the behavioral sophistication allied to pharmacological and neurophysiological competence characteristic of Weiskrantz and of S. Iversen flourishes still at Cambridge, primarily through the work of T. W. Robbins. Although Robbins is also predominantly a neuropharmacologist, he has made important contributions to behavioral pharmacology, as, for example, in his critical analyses of the rate-dependency hypothesis of drug action (e.g., Robbins & Evenden, 1985). Robbins has also proved to be supportive of behavioral pharmacology in the behavior-analytic tradition during his distinguished editorship of the important journal *Psychopharmacology*. With Weiskrantz as head of the department, behavioral neuropharmacology has also flourished at Oxford University in a diversity of ways. Perhaps the work of most relevance to the present context has centered on the research related to J. A. Gray's theoretical and experimental analyses of anxiety (e.g., Gray, 1982). Gray is now based at the Institute of Psychiatry in London.

There remain other researchers in Britain whose contributions to behavioral pharmacology must be included even in a brief review such as this, though their interests or pedigree cannot perhaps be traced so readily to the Ciba symposium. For example, J. C. Leslie is a noted behavior analyst who originally worked with Millenson at Oxford. Their behaviorally oriented introductory textbook (Millenson & Leslie, 1979) needs no emphasis here, and Leslie, now head of the Psychology Department at the University of Ulster in Northern Ireland, has sustained a regular output of research reports in the experimental analysis of behavior, sometimes with his own former graduate students. Some of this work has included the investigation of drug effects, as, for example, his study of the effects of *d*-amphetamine on response patterning in fixed-interval schedules (McAuley & Leslie, 1986). Another regular contributor to the literature of the experimental analysis of behavior is C. M. Bradshaw. Bradshaw is a remarkable research scientist, in that with his collaborators Szabadi and Bevan he has sustained active and productive research programs not only on mathematical models of response allocation in animals and humans

(e.g., Bradshaw, Szabadi, & Bevan, 1978) but also in basic neuropharmacology (e.g., Szabadi, Bradshaw, & Bevan, 1978). It is not surprising, then, that Bradshaw and his colleagues should have included drugs in some of their behavioral studies, as, for example, in Ruddle, Morley, Bradshaw, and Szabadi (1984), in which the effects of pentobarbitone were investigated on variable-interval behavior in rats in the context of Herrnstein's equation for response allocation.

Finally the work of A. J. Goudie of Liverpool University commands attention. With his colleague Demellweek, Goudie has made major contributions to the study of the behavioral mechanisms involved in the development of tolerance to the effects of amphetamine (see review by Goudie & Demellweek, 1986). Goudie has also made some exceptionally sensitive experimental and theoretical analyses of conditioned taste aversions and the stimulus properties of drugs (e.g., see Goudie, 1987).

Before drawing this review to a close, it is interesting to note that many of the researchers whose work has been mentioned above have also played important facilitative roles in the development of behavioral pharmacology by editing volumes of international research reviews. Blackman and Sanger (1978) edited an overview of research in the behavioral pharmacology tradition, with contributions from leading researchers of international standing including McKearney and Barrett, D. M. Thompson, Winter, Johanson, and Evans and Weiss, as well as chapters by Stoleran and the expatriate Gilbert. Sanger and Blackman (1984) also edited a more general introductory text that included contributions from Barrett and Stoleran and the expatriate Greenshaw. Stoleran edited, with Goldberg, an important collection of papers on the behavioral analysis of drug dependence, which included contributions from Colpaert, Goudie and Demellweek, Katz and Schuster, and the expatriate Sanger (Goldberg & Stoleran, 1986). Goudie and Emmett-Oglesby (1989) edited an authoritative collection of papers on tolerance and sensitization to the effects of drugs that included contributions from Wolgin, Siegel, Barrett, and Blackman. A book on the neuropharmacological basis of reward edited by Liebman and Cooper (1989) also deserves mention here,

for it, too, includes papers of considerable behavioral sophistication, for example, by Katz and by Bradshaw and Szabadi. S. J. Cooper of Birmingham University, now also a Professor of Psychopharmacology, provides an example of a distinguished British behavioral neuropharmacologist who, although not favoring the behavior-analytic tradition, has also made a major contribution to the study of the effects of drugs on behavior. Other British researchers who might be mentioned in this vein are I. Hindmarch of Surrey University, D. Warburton of Reading University, and P. Willner of the City of London Polytechnic, all of whom have published extensively, including books that have been well received in neuropharmacology. Willner in particular also plays an important supportive role for behavioral pharmacology in the behavior-analytic tradition through his foundation editorship of the journal *Behavioural Pharmacology*, which was launched in 1989 and which promises to become a major outlet of international standing for this approach. Willner's powerful editorial board includes the following British researchers who have been mentioned in this brief review: Sanger (expatriate), Cooper, Goudie, Gray, Robbins, Stolerman, and Warburton.

How is a brief and selective review such as this to be ended? Inevitably with a further apology for omissions or inappropriate emphases. I have tried to review the historical development of a British contribution to behavioral pharmacology in the behavior-analytic tradition, but my interpretation of the theoretical position of others may not be their own. A similar review with a more neuropharmacological orientation has recently been provided by Robbins and Stolerman (1990). It cannot, however, be doubted that since 1964 British-based researchers have made substantial contributions to the development of behavioral pharmacology. It may seem churlish to have suggested earlier that the historical development of behavioral pharmacology in Britain has been faltering. The researchers discussed above are determined and robust, effective in sometimes quite unsupportive conditions. But I am struck by a number of constraints that have hindered systematic development of this field in Britain. The first is posed by the relatively unsym-

pathetic climate of British psychology with respect to behavior analysis in general, which has a consequent effect on the evaluation of behavioral pharmacology in the behavior-analytic tradition. A second constraint on systematic growth has been posed by the very success of a more neuropharmacologically oriented approach to the study of drug-behavior interactions in Britain. A third constraint has been the consistent loss of considerable talent to laboratories in other countries, prompted largely by the difficulty of establishing research facilities with some degree of critical mass and with relatively secure funding. Yet a further general constraint, sadly shared by colleagues in other countries, has been the increased moral and financial pressure on nonhuman-based experimental research in recent years. A review by Thomas and Blackman (1991) has shown that on a number of measures, nonhuman work in British university departments of psychology had decreased by 1989 to a value only one third of the baseline measured in 1977.

So, although there is reason for considerable pride in the contributions of individual British researchers to the development of behavioral pharmacology, there is also some feeling of regret at what might have been accomplished with a fairer wind blowing.

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